

known that the double varieties, those usually cultivated and, therefore, most likely to be gathered, are neutral, having neither stamens nor anthers, and consequently no pollen. It is a singular fact, pointed out by Dr. Wyman in his essay on *Autumnal Catarrh* (New York, 1872), that persons in New England who have "rose cold" or hay fever there in June, have escaped a similar affection at the same season of the year in England, notwithstanding that the grasses which are said, by our author, to furnish 95 per cent. of the effective pollen found in the air, according to Dr. Gray, our most eminent American botanist, are botanically identical in the two countries. Again, many of the flowers and other agents, which produce the symptoms of hay fever at the critical season of the year, are apparently powerless at other seasons, although, while in full bloom in conservatories, they are visited by persons who had previously suffered. The evidence in favour of other exciting causes than pollen does not seem to us quite fairly weighed. The dust of the highway, so annoying to hay-fever patients, Mr. Blackley assures us is only thus annoying because of the pollen which is mixed with it, although he also assures us that the effects of pollen are partly mechanical, and that a certain amount of pollen may exist in the air without appreciable symptoms. So again, the dust and smoke in a railway carriage, which almost invariably at the proper season bring on a paroxysm even when there are no hay-fields near, are, he thinks, wrongfully suspected; the true cause is the pollen brought in the carriage from regions where grass is in flower: "That this is not a matter of speculation," says Mr. Blackley, "my own experience abundantly proves." Until he shall have given us the method by which he distinguishes the effect of the dust from that of the pollen, he must permit us to deem his explanation farfetched. Notwithstanding the evidence of the controlling influence of theory, and a disposition to speculate and generalize on too narrow a basis of facts, the reader will find in this book much that is new and instructive.

M. W.

ART. XXVII.—*Die Diagnose der Eierstocks Tumoren, besonders der Cystomet.* VON OTTO SPIEGELBERG. Sammlung Klinischer Vorträge, No. 55.
On the Diagnosis of Ovarian Tumours. A clinical lecture. By OTTO SPIEGELBERG, March, 1873.

THIS lecture belongs to a most excellent series now in course of publication in Germany; a course of clinical lectures upon practical subjects, by eminent men, in every branch of the profession, chosen from every part of the country. The one before us bears the name of a renowned obstetrician and gynaecologist, and is of especial interest to us at this time because following closely upon the publication, in England and this country, of important works upon ovarian disease and ovariectomy which have been reviewed in our pages, and also because showing that Germany, late as she has been in taking up this important advance in our art, has already close and thorough students of it.

The lecture opens with the relation of two cases of pelvic tumour. In regard to the nature of one there was a difference of opinion, which neither the chemical nor microscopical characteristics of a specimen of its contents, withdrawn by puncture, could reconcile; but a diagnosis of ovarian cyst was finally made by resorting to Simon's manual examination *per rectum*, and confirmed by the operation of ovariectomy. In the other, all the usual measures having been exhausted, an exploratory incision was resorted to, and the feasi-

bility of the removal of the tumour negatived. Eight weeks afterwards the patient died, and it proved difficult even at the post-mortem examination to make out its sole origin from the uterus, its implication of the various pelvic organs, and the non-participation of the ovaries in the morbid growth.

These cases furnish the text for a lecture upon the diagnosis of ovarian tumours, which is, of course, neither methodical nor exhaustive, but which presents some points of interest deserving of mention.

In settling the first question which presents itself upon commencing the examination of a case, viz., "does a tumour exist?" he states the well known application of chloroform as a means of diagnosis, but where persistent tympanites causes the error, says that he attains his end more readily by *persistent and firm pressure upon the abdomen*, a plan already recommended by Ræderer. The patient lying on the back with limbs drawn up—

"I exert a steady pressure towards the spinal column with both extended hands; as, with expiration, the parietes of the abdomen relax, I press with all my strength, hold what I have gained against the next inspiration, and so continue the manipulation, and the resistance of the patient is soon overcome. By this plan I have already convinced many esteemed colleagues and many patients of the groundlessness of their fears."

After detailing the differential diagnosis between ascites and ovarian cyst he alludes to those cases in which exceptional conditions are present and may mislead. In some of these cases he has found assistance from a rule first laid down by the late Prof. Breslau of Zurich:—

"In a cyst the fluctuation ceases where the intestinal resonance begins (loins and epigastrium), for both are bounded by the sac-wall; but if the fluid moves freely through the abdominal cavity, if it passes between the coils of the intestines (ascites), then fluctuation will be perceived even in places where percussion gives the intestinal resonance."

But, all other means of diagnosis having failed, puncture and examination of the fluid withdrawn is to be resorted to and is a measure in which the author has the highest confidence, and one which he uses in all cases of doubt. He admits that it is not without its dangers, but says nothing of two fatal cases attributed to him by Peaslee. It is a measure, too, which he believes is not estimated by authors as highly as it deserves, and for proof refers to Spencer Wells's "lately published and otherwise so excellent book;" and to Peaslee's "perfectly equally valuable work." Atlee alone, of all the later authors, he believes duly esteems the value of the diagnostic puncture; yet, he says, "the description he gives (pp. 57-60) and particularly in the twenty-fourth chapter, from Drysdale (pp. 442-469), do not reach the level of our German investigations."

In his detail of the organic substances found in ovarian cysts he seems to follow Eichwald, as does Wells, and lays particular stress upon the presence of paralbumin, a substance never found in ascitic fluid. He emphasizes the value of the morphological part of the examination, and enters on it particularly "on account of misconceptions of the subject," referring again to Wells. His diagnosis from these substances rests on the presence of the endothelium of serous membranes in the one and the epithelial elements in the other; on the character of the cells furnished in accordance with the parts from which they were developed rather than on any particular cell which can be looked upon as characteristic of either fluid.

"You see therefore that peritoneal fluids furnish the elements that are to be expected from a lymph-sac, cysts those of epithelial formation. This distinction is decided, and where it is clearly found, decisive. I refer you to the case of F.; cylindrical cells were the only bodies found in favour of an ovarian cyst,

and this the operation proved it to be. I have published similar cases in my archives, and I have been enabled several times, from the simultaneous appearance of lymphoid and epithelial cells, to make a diagnosis of communication of the cyst with the abdominal cavity, or the coexistence of ascites with cysts."

He then places in juxtaposition the characters of the two fluids, but we find no mention of any one peculiar cell as characteristic of ovarian cysts, or the discovery of any one body alone relied upon for diagnosis.

The great and surpassing difficulty of diagnosis which lies between ovarian and fibro-cystic tumours of the uterus is well known. The author recognizes it clearly and acknowledges that there are cases likely to deceive the most experienced investigator. He alludes to the spontaneous coagulability of the fluid of uterine cystic disease, as given by Atlee, but has had no opportunity of verifying the observation. In these obscure cases two resources remain; an exploratory incision and the introduction of the hand into the rectum, as advised by Simon. The paper of Simon was noticed in this Journal for April, 1873, p. 548, and the procedure, which of course takes place under complete anaesthesia, is there described. We give the author's experience:—

"In six cases in which I found this measure necessary I succeeded in the introduction of the hand without difficulty, and without division of the raphe, only one or two very superficial incisions being made in the anterior margin of the anus. In one patient, a young woman who had never been pregnant, the incision tore through the sphincter; the wound healed by granulation and in twelve days full power was regained; in the other cases all was right in three or four days. A thorough knowledge of the topography of the pelvic organs is, of course, necessary in order to be able to distinguish parts with the hand, thus in the rectum and fingers extending upwards; but whoever has never yet availed himself of this method, cannot imagine how thoroughly the pelvic cavity can be explored, and changes in size or position of its organs recognized. Even the character of a tumour may sometimes be decided by it, as you saw in F.'s case, for the segment of the cyst recognized *per rectum* decided the diagnosis. In other cases you will be able to feel the very origin of the tumour and thus make an absolute diagnosis, or, again, by mere negative results, do this *per exclusionem*. Thus, in a case where it was doubtful whether the tumour was ovarian located in front of the uterus, or a fibroid springing from this organ, several *cliniciens* had given different opinions, and I myself could not by the ordinary mode of investigation arrive at a definite conclusion. I felt directly the origin of the pedunculated sub-peritoneal tumour from the fundus of the uterus, and therefore prevented the desired and intended ovariectomy."

To the last diagnostic resort, exploratory incision, Dr. Spiegelberg has had recourse four times. In one ovariectomy followed immediately; in the second he discovered an echinococcus cyst behind the peritoneum on the left side, cut out a portion and stitched the remainder to the wound; the patient recovered; the third was one of the cases detailed, the patient dying after a couple of months; in the fourth, the tumour was found not to be removable and the wound closed, the patient suffering only a slight attack of local peritonitis.

The lecturer has found the diagnosis of renal cyst, when long-standing and of large size, the most difficult task of all, and refers to a publication by one of his pupils¹ for the detail of numerous examples of errors, and of one case by himself. He refers to the eight propositions laid down by Spencer Wells upon this subject, and says:—

"But neither the location of the tumour, nor the situation of the intestines in relation to it, nor the disturbances of function, are, in advanced stages, abso-

¹ Diagnostische Irrthümer, welche bei ovariectomien vorgekommen sind. Breslau, 1867.

Intely reliable. I have shown that a renal tumour may be in a median position, and that it may extend into the pelvis like an ovarian tumour; and there may even be, as in Baum's and Dumreicher's cases, adhesions to the pelvic organs; I have also emphasized the fact that such tumours may have the intestines over them or on the side of them, they being pushed aside or the mesentery even perforated, and it is well known that ovarian cysts sometimes have intestinal coils in front of and under them. Tumours of the kidney may be movable and those of the ovary immovable; the original location of the growth is only to be discovered in the early stages, and then perhaps is only made by accident. The puncture will enable us to decide in many cases, by giving proof of the presence of the epithelial forms peculiar to the ovaries, or the renal elements, such as the sediments of the urine, urea, echinococci (for this parasite has never yet been met with, so far as I know, in the ovary); but even these may deceive, for urinary constituents are not always found in the contents of nephritic cyst, and may perhaps be found in the ovarian (Hanauyn found allantoin), even echinococci may be present and furnish no evidence of their existence; and on the other side, again, cholesterin and paralbumin have been found in the kidney cysts. In such circumstances, the manual examination *per rectum* is of the greatest value, for by it can at least be proved whether the cyst has its origin in the pelvis or not, and by this help exceedingly to clear up the diagnosis, even when the origin itself cannot be reached."

Possibly we have occupied too much space with this examination of a single clinical lecture. If so, we plead in excuse, the high authority from which it emanates, and the interest attaching to a subject not yet entirely mastered and which will always present to the diagnostician his severest problems.

J. C. R.

ART. XXVIII.—*Die Kaltwasserbehandlung des Typhus Abdominalis nach Beobachtungen aus der Medicinischen Klinik zu Erlangen.* Von Dr. HUGO ZIEMSEN, O.Ö., Prof. der Spec. Pathologie u. Therapie, Director der Klinik, etc., und Dr. HERMANN ZIMMERMANN, Assistentarzt, etc.

The Cold-water Treatment of Typhoid Fever, according to Observations in the Medical Clinic in Erlangen. By Dr. HUGO ZIEMSEN, Prof. of Special Pathology and Therapeutics, Director of the Medical Clinic and Polyclinic, and Dr. H. ZIMMERMANN, Assistant Physician, etc.

THE practical object of this book is to show the effects of cold water upon typhoid fever; to furnish statistics; to lay down exact methods of using cold water in this disease; to present statements of careful study of temperature under its use, and to afford proofs of the success which the authors claim to have followed upon the mode of treatment in question.

In 1861 Ernst Brand recalled the attention of the medical public to the use of cold water in typhoid fever, first adopted by Currie and his disciples. Following Brand, Ziemssen, Gietl, Niemeyer, Bartels, Jürgensen, Liebermeister, and Hagenbach in their respective clinics, entered upon a systematic use of hydro-therapeutics in typhoid fever, each and all becoming converted to the usefulness of the treatment, besides agreeing in the main in regard to the comparative utility of the various ways in which cold water was applied. Ziemssen first adopted Brand's method, that of applying douches to the upper half of the body of the patient, whose lower half was immersed in a bath of cold water. Brand recommends this as the most effectual process. Ziemssen soon discarded Brand's procedure, and passing through other modifications of this treatment, finally settled upon the "graduated full bath," the results of which are given